NEW CONCEPTS FOR FAR–INFRARED AND SUBMILLIMETER SPACE ASTRONOMY

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ABSTRACT

The "Second Workshop on New Concepts for Far-Infrared/Submillimeter Space Astronomy" was a forum for community input into the NASA Space Science strategic planning process. The participants discussed science goals, mission concepts, and enabling technologies for future space observatories that will operate in the 20 - 800 micron wavelength range. The Workshop was motivated by the Decade Report's assignment of priority to a Single Aperture Far-Infrared (SAFIR) telescope and encouragement of the subsequent development of space-based far-infrared interferometry. A white paper, currently in preparation, will give the community's recommended science and technology roadmap for space missions in the post-SIRTF, SOFIA, and Herschel era. The roadmap is intended to guide the implementation of the Decade Report recommendations[1]. In this talk I present a review of the conference and a preview of the white paper.

INTRODUCTION

The far-infrared and submillimeter bands are extremely rich in information for space science studies. Topics that can be uniquely addressed in this spectral region range from nearby star and planet formation to the evolution of the very oldest structures in the Universe. Compared to other wavelengths, in the far-infrared and submillimeter the atmosphere is so opaque, most current telescopes are so bright, and current detectors are so underdeveloped that the potential of future space missions for answering fundamental questions in this spectral region is tremendous. However, significant advances will only be enabled by space missions which, in turn, are enabled by certain critical technologies. This is the context of the Second Workshop on New Concepts for Far-Infrared and Submillimeter Space Astronomy, held in College Park, Maryland on March 7-8, 2002.

The purpose of the workshop is to provide a broad base of support for ideas to be injected into NASA's roadmapping effort. The goal is that these ideas should suggest top priority science investigations, missions to tackle those questions, and technology investments to enable those missions. After the workshop, a proceedings documenting the entire invited and contributed material is to be published as a record of the state of the far-IR and submillimeter space astronomy community. Additionally, a comprehensive summary document, preliminarily entitled "The Community-Endorsed Roadmap for Far-IR/Submillimeter Space Astronomy", is being prepared as one of the inputs into NASA's roadmap[2].

The workshop was attended by 130 participants from 50 institutions, and represented scientists and engineers from many countries and with a wide variety of experience. The technical content featured 17 invited talks and 44 contributed posters, complemented by two six-person panels to address questions of astronomy and technology.

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Figure 1. Participants at the workshop.

STRATEGIC PLANNING

The NASA strategic planning effort is an involved process that attempts to distill the ideas of thousands of professionals into a clear plan of action with far-reaching influence. As a part of this process, several workshops are held as a grass-roots accounting for these ideas. This includes the New Concepts workshop as well as the workshop to which this summary is being submitted. Because of the importance of the strategic planning exercise, presentations on this topic were given by Phil Crane[3] (NASA HQ), Dan Lester[4] (UT Austin; member of SEU roadmapping committee), and Harley Thronson[5] (NASA HQ) at the New Concepts Workshop. Below is a summary of these presentations.

For the Space Sciences Enterprise, NASA defines four subcommittees as its advisory structure: Sun-Earth Connection (SEC), Astronomical Search for Origins (ASO), Structure and Evolution of the Universe (SEU), and Solar System Exploration (SSE). The process of NASA strategic planning starts with the definition of questions to be addressed – in this case, originating in documents such as the Decade Report of the National Academy of Sciences[1]. Roadmap teams use these documents to produce a roadmap with a 10-20 year horizon. The Strategic Plan for the Space Sciences Enterprise is derived from the roadmap, with a horizon of about 5 years. The next Strategic Plan will be written in November 2002, and is updated every 3 years. The rationale for such repeated long-range planning is to keep visibility into NASA's planning process for Congress and, by extension, the public.

It is important to note that the Strategic Plan (and the comprehensive Plan for the entire agency) is used to evaluate the performance of NASA, thereby providing validation that funding produces results. In addition to being used after the fact as a metric, the Strategic Plan serves to focus investments in science and technology in order to achieve the outlined goals. It is this aspect of the Plan that motivated the New Concepts workshop: missions in the roadmap imply certain technology and concept development in the Strategic Plan; the Plan can be used to justify requests for financial support for these development items.

The present situation is not without some preexisting structure. The ASO plan (the Origins program) has already sold a line of missions to Congress, so its roadmap for this decade will remain fairly static. In contrast, the SEU plan has not yet been sold, so opportunities for new missions reside there. However, the two missions currently in the SEU docket – Constellation-X and LISA – are going to be the near-term centerpieces of the SEU roadmap, and so selling those missions to Congress is critical to securing funding for all other SEU missions. At the moment, community input from a variety of sources such as the NASA-funded workshops (e.g., the New Concepts workshop and the Far-IR Detectors workshop) and a set of voluntary White Papers will be able to influence the mission suite sold in the SEU plan. However, since the Decade Report suggests specific mission priorities, these will likely dominate.

The SEU committee report will be used to demonstrate that a plan has been formulated with the consensus of a large number of scientists, and that they have made hard choices about priorities. The highest science priorities will be used to make a mission line. This approach has its disadvantages: the mission line is based on the present short horizon view of high science priorities, and mission lines – once sold – are a somewhat inflexible suite of science experiments. During this roadmapping exercise, the community should think out of the box about new approaches: focused science programs can be defined with missions chosen later; or an initiative for a far-infrared mission without regard to specific science investigations could be made; the division of ASO and SEU – which seems to divide the infrared community in half – can be removed. One other idea – which has garnered much support due to its flexibility and the scale of opportunity – is to expand the Explorer mission line with a "BigEx" mission to complement the SmEx and MidEx. This mission product, available perhaps for one launch per two years, would have a cost cap at about $300M (comparable to Discovery) and would fill in the gap between the MidEx (currently $180M) and strategic missions (which tend to be ~$500M or more).

Technology development is a portion of NASA's strategic plan that occurs well before roadmapped missions. Near-term (within 10 years) technology development is primarily mission-specific, and so is mission-managed. Longer term technology development funding derives from missions in the roadmap. It is important that long-term funding be maintained (despite the ever-present near-term crises) so that technology is developed early and demonstrated in laboratory, ground-based, or suborbital environments. NASA is a conservative agency, and flight demonstrations are few, so technology is not easily infused into missions.

TECHNOLOGY NEEDS

Future missions can be predicted, and their technology needs estimated. After SIRTF, the far-infrared community will need a larger cryogenic telescope for better angular resolution, lower confusion noise, greater sensitivity, and an increased wavelength range. This mission is suggested in the Decade Report as SAFIR[6]. Eventually, the study of distant galaxies in detail will require still higher angular resolution, such as is only achievable with a space-based interferometer such as SPECS[7]. At longer wavelengths, an exquisitely sensitive all-sky survey of the polarization of the microwave background will be necessary for probing the physics of the very early Universe. Such missions, and smaller competed missions, will require cryogenic, optical, and detector technology development.

In order to reduce emission into these telescopes, all of them will need radiative cooling (including sun shielding) to permit temperatures as low as 30K. Closed-cycle refrigerators to ~6K will be needed for cooling optics for some of these missions, and lower temperature (<1K) refrigerators such as continuous ADRs[8] will be required to cool detectors for most of these missions.

Optics will need to be larger, lighter (i.e., lower areal density), and be much colder (~4K) than those on NGST. Fortunately, the optical figure requirement is much less demanding at far-infrared wavelengths. Additionally, the technology for long baseline space-based interferometry (a complex system of optics, control and metrology, and spacecraft attitude and formation control) will ultimately be required.

Detectors may be the greatest challenge, in that there are no other sources of funding or development effort. Far-infrared detectors do not have commercial or military support, and are necessarily specific to the space-based environment. Cryogenic telescopes in space have photon backgrounds orders of magnitude smaller than are seen from even airborne platforms. There may be some possibility of leveraging superconducting bolometer development with similar efforts underway for the development of microcalorimeters for Constellation-X. For direct detectors, future missions will require large format (1k+ pixel) arrays of background-limited detectors over the 20-800μm range. While the MIPS 70μm array has achieved this[9], photoconductors do not yet work beyond 200μm, so large format bolometer arrays will likely be the technology developed to meet this requirement. Coherent receivers with low noise, broad bandwidth, capable local oscillators, and compact backend spectrometers will have to operate over bands ranging from ~400GHz up to ~4THz. The challenges facing detector builders are not so much the fabrication of lower

noise detectors, but rather of building large scale, robust, capable systems. Substantial engineering efforts will be needed to enable this kind of detector system for space flight purposes.
One additional concern that touches on all the above component technologies is the issue of the infrastructure for developing space flight technology. Manufacturing capability is a problem for the highly customized products NASA will need for far-infrared missions; maintaining this capability from early research through space flight is difficult. Furthermore, the testing facilities for systems optimized for space (such as very large optics or very low background detectors) may be difficult to implement on the ground. It is therefore crucial that flight heritage be used whenever possible, and the flight opportunities be made available. SOFIA, serving as a halfway point between ground and space, may provide some opportunities. Explorer-class missions should also accept higher risk, higher return technologies so as to enable future strategic missions.

IMPORTANT STATEMENTS

In this section, I will attempt to summarize what I feel are a number of relevant, important, or pithy statements that were made during the workshop. At risk of slandering others, I will identify the speakers; however, my transcription may have inadvertently altered the context of these quotes. All ideas should be credited to their originators, and all mistakes to me.

Strategic Planning for the Far-IR

Hashima Hasan stated that she wants the community to think about what research should be done in space in the far-infrared. In order to provide a venue for this thinking to take place, she has set up working group to distill ideas under the aegis of NASA headquarters and organized by Eric Smith. It will serve as a conduit between the far-IR community and NASA.

In a cautionary note, Martin Harwit (rightly, I suspect) observed that Congress is not likely to endorse many new starts. Very few new missions will appear in the coming decade. If one of these missions is to be a far-IR mission, then the far-IR community will need to articulate to peers in astronomy observing at other wavelengths what this mission is and what benefit it will have for all astronomy. If it is time for a far-IR mission, we in the far-IR community must be prepared to be articulate.

Far-IR Mission Roadmap

NASA operates on a mission-line approach. A sequence of missions is planned for and is presented on the roadmaps; these missions are initiated and flown in order until the mission line is complete. At the moment, there are no major far-IR missions beyond this decade in a mission line, awaiting the start of funding. How do we get missions injected into the roadmap? What is the endpoint of this mission line? For the Origins program, TPF (Terrestrial Planet Finder) is the distant goal, with a clearly identified science goal (finding terrestrial planets). Does the far-IR have an equivalent?

Overall, comments indicate that this endpoint is beyond our horizon. Harvey Moseley summarizes the situation this way: in order to make any progress, we have to have a path of missions; the destination will become clear with time, but the science goals must be compelling along the way. Gary Melnick agrees, stating that being too specific early on can engender a tunnel vision (as happened in the 1980s with LDR) which can result in a lockout of more feasible alternatives. Charles Lawrence points out that, like TPF, likely missions in the far future will be exceedingly ambitious and may appear to be beyond our grasp at the present. John Mather suggests further moderation, in that only a handful of top science goals should be raised – those most prominent and promising – and that specific recommendations must flow into roadmap from these.

Detector Technology Planning

of direct detectors since his first device over 40 years ago through to the present. The capability (defined as the speed of mapping to a given noise level) has increased exponentially, obeying Moore’s law by doubling roughly every year. In order to meet the requirements of future far-infrared missions, at least five more doublings are required. In this case, it may be necessary to neglect evolutionary developments of

current technologies, but rather to enable revolutionary developments by investing in more extensible technologies such as multiplexed superconducting bolometer arrays.
Coherent detectors – notably the SIS receiver – have undergone substantial improvements, such that the <1THz region has receivers with noise temperatures only twice the quantum limit. Jonas Zmuidzinas[11] observed that, particularly for broadband and high frequency (where near quantum-limited receivers are yet to be made) applications, the best coherent detectors will add excess noise as compared to the low photon background present in space. However, the advantage of very high resolutions – parts per million – are not easily achieved with any implementation with direct detectors.

Since the capability of far-infrared missions is so dependent on the detector technology available, it will enable missions rather than the other way around. As John Mather put it, "everything revolves around detectors, so they must be developed first."

Science Investigations

The science discussion panel gave a broad introduction to the scientific topics addressed by mid-infrared to submillimeter investigations. Many of these – notably star formation, planetary system formation, AGN and starburst diagnostics, and early star/galaxy formation - cannot be studied conclusively without investigations at these wavelengths. Additionally, several of these projects will ultimately require a combination of both high spatial and spectral resolution in order to study them.

Mike Werner[12] first made the observation – bolstered later by others – that polarimetry in the far-infrared and submillimeter is scientifically important, but that no generally available capability currently exists for this. Possible near-term implementations of polarimeters might include a SOFIA polarimeter or even a polarimetric explorer mission (perhaps implemented as part of a sky survey mission[13]. An all-sky survey of Galactic polarization may be essential as a scientific precursor to a CMB polarization anisotropy measurement mission.

Jean-Loup Puget[14] showed models of star and galaxy formation at high redshifts. The models indicate that the hierarchical assembly of galaxies implies that early objects might appear as clusters of low-luminosity objects with an observed-frame SED such that they could be missed by both NGST and ALMA. An all-sky far-infrared survey would be necessary to detect them.

Daniela Calzetti[15] made the interesting observation that scientific investigations in the UV and optical tend to be limited by systematics. This is also very much true of CMB anisotropy investigations. However, in the mid-infrared to submillimeter, the limits are a combination of our poor understanding of the physics of emission mechanisms and the relatively primitive observing tools we can bring to bear on these problems.

Crystal Ball Gazing

This is a term borrowed from John Mather, who evaluated the current situation in astronomy[16] and made the following statement (edited to fit, and with some of my own observations):

> Based on the principle that we must do everything simple and cheap before we can do anything complicated and costly, there's a logical progression of far IR projects to do.
>
> 1. A wide field (probably all-sky) Far-IR survey – launching before 2010, since it is technologically more realizable and can improve the scientific return for later missions.
> 2. A Cosmic Microwave Background Polarization mission, at about the same time.
> 3. A Single Aperture Far-IR telescope, SAFIR, as advocated by the Decadal Survey – launch about ~10 years away.
> 4. After SAFIR, to beat the confusion limit at wavelengths >100µm, either an imaging interferometer or a new telescope 3 times as large – flown around 2020.

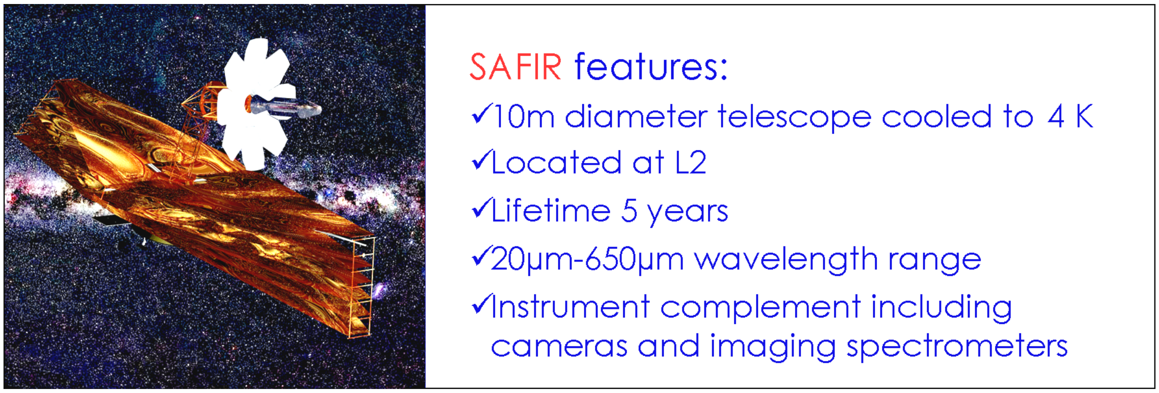

Figure 2. Possible SAFIR implementation and some baseline parameters.

CONCLUSION

The Second Workshop on New Concepts for Far-Infrared and Submillimeter Space Astronomy canvassed the community for ideas about science investigations, the missions to pursue them, and the technologies to enable the missions. Five significant results following from the conference are:

- NASA will support a Far-IR Science Working Group.
- Certain technologies (cryogenics, optics, and detectors) will require substantial NASA support.
- NASA should support the mastery of interferometry in this decade.
- NASA should implement a larger class of space sciences mission in the Explorer line
- Workshop participants recommend SAFIR as *the* high priority mission.

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